HESS Discussions, 10, 15219–15262, 2013

Challenges in conditioning a stochastic geological model of a heterogeneous glacial aquifer to a comprehensive soft dataset

Correspondence to: J. Koch (juko@geus.dk)

Author comment to the Editor: Prof. Mauro Giudici.

We would like to thank the editor Prof. Mauro Giudici for handling the revision process of our manuscript. We are very pleased that a scientific improvement has been acknowledged after our first revision by both, the editor and the reviewers. The fact that the editor has attested minor revisions to this iteration of revision makes us confident that our manuscript, after thorough revision following the suggestions, is soon fit for publication.

> *The Referees disagree with some of the results and comments expressed by the Authors, but they rate the paper as good and think that it deserves publication. I agree with them and ask the Authors to provide a minor revision, by taking into account the specific comments by Referee #1.*
> *With respect to my personal comment on the relationship of electrical resistivity with hydraulic conductivity or with sediments and pore fluid properties, I draw the Authors' attention to some papers, whose DOI are listed below. See also the reference lists of those papers and the citing papers for a wide, even if incomplete, review.*

The suggested literature was useful and inspiring for further elaboration on the integration of geophysical and hydrogeological data. As this is not the main focus of our study we placed the added section not in the general discussion but in the "Data" section.

The following paragraph was added to section 3.3 in the revised manuscript:

The HPM-method is a probabilistic approach and it is preferred to deterministic model approaches, because of its suitability for soft conditioning in a stochastic geological simulation. In a more deterministic sense, a positive correlation between resistivity soundings and hydraulic conductivity estimates derived from pumping test has been acknowledged for glacial outwash aquifers (Urish, 1981). Linde et al. (2006) review different strategies to relate geophysical and hydrogeological properties and attest that geophysics undoubtfully add value to a hydrogeological characterization. One suggested approach, which would be suitable for SkyTEM data for the estimation of hydrogeophysical parameters, is the joint inversion, where the geophysical or the hydrogeological inversion utilizes hydrogeological or geophysical data, respectively. Another review paper by Slater (2007) address joint inversion methods as well as petrophysical relations between geophysical (electrical) properties and effective hydraulic properties (pore volume and pore surface) at core and field scale, which allow direct mapping of hydraulic properties (Kemna et al., 2004). Slug tests with an estimate of the local saturated hydraulic conductivity are available for the Norsminde catchment. However, due to differences in scale, a low number of slug tests and unclarity of the correlation we found the direct mapping approach unfeasible for our study.

Kemna, A., A. Binley and L. Slater, Crosshole IP imaging for engineering and environmental applications, Geophysics, 69(1), 97-107, 2004.

Linde, N., J. S. Chen, M. B. Kowalsky and S. Hubbard, Hydrogeophysical parameter estimation approaches for field scale characterization, SPRINGER, DORDRECHT, 2006.

Slater, L., Near surface electrical characterization of hydraulic conductivity: From petrophysical properties to aquifer geometries - A review, Surveys in geophysics, 28(2-3), 169-197, 2007.

Urish, D. W., Electrical Resistivity-Hydraulic Conductivity Relationships in Glacial Outwash Aquifers, Water Resources Research, 17(5), 1401-1408, 1981.

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Author comment to review #1: Anonymous Referee.

We would like to thank the anonymous referee #1 for her/his review of our revised manuscript. We are very satisfied that the she/he acknowledges the scientific improvement of our revised manuscript and is willing to accept submission after minor revision. The additional suggestions she/he poses will be taken into account and the manuscript will be corrected accordingly. We enjoyed the scientific dialogue and are further very pleased with the increase in scientific quality of our manuscript.

> *This revised version of the manuscript is definitively an improvement compared to the previous one, although I feel many of my previous comments have not been entirely addressed. The limitations of the method are however discussed and essential background information is given.*
> *My main criticism towards this work is that it is based on the problem that the authors call "overconditioning" and designing ways of solving it. The fact is that overconditioning is only a problem when the exhaustively informed variable (SkyTEM in this case) has a different nature than the target variable (here the facies), and when this difference in nature is not taken into account properly. Classical cokriging approaches explicitly model the relationship between the spatial structure of the different variables by using cross-variograms. The authors do not model this bivariate relationship, and as a consequence they deal with an inadequate spatial model of variability. Then they solve the problem that they have created themselves by using a data decimation strategy which removes the structural influence of the soft data.*
> *In the end, the strategy works and seems to apply in the context of Tprogs, hence it might be a valuable contribution for the users of this program. However I do not clearly see the advantage compared to other commonly used techniques, such as to use the resistivity information to derive a probability map for the occurrence of a given facies, which is then used as soft conditioning or a trend.*
> *I also believe that a comparison of the proposed decimation scheme with usual soft probability conditioning method (such as collocated co-simulation for example) would be very helpful.*

At first, we had different understandings of the above mentioned comment by the reviewer and how to approach it. We understand that her/his main point criticizes our data integration/modelling between geophysical data and facies, because it does not take the differences/disagreement in spatial nature into consideration. Following, this disagreement causes the problem of overconditioing in the TProGS simulation. We do not agree with the reviewer at this point and want to discuss "overconditioing" from to angles in the following.

The discussion of the overconditioing problem has two dimensions. The first, as pointed out by the reviewer, concerns how to model the relationship between geophysical data and facies data correctly by considering the differences in spatial nature of the two variables. We agree that we do not directly consider the difference in spatial nature between the geophysical data (SkyTEM) and the target variable (facies); only indirectly in the Histogram-Probability-Matching method (see below). Yes, cokriging with cross-variograms might a more adequate approach to directly model the bivariate relationship. However, we chose an empirical approach where the relationship between the geophysics data and facies data is handled outside the stochastic simulations. The shape of the fitted curve (flatness) represents various lumped sources of uncertainty; one of them is the difference in spatial nature between the two variables. In our study we do not disaggregate the lumped uncertainty, because that is not the focus of the study. Therefore we believe that the issue with different spatial natures using different sources of data has been considered; but in an implicit way.

The second dimension is more TProGS specific and deals with the question if continuous soft data is treated accordingly by the simulation. We experienced that TProGS generally has problems with exhaustive soft data. Overconditioing in TProGS arises because the SIS and the simulated quenching algorithms integrated in the current version do not handle soft data, which are extremely spatially correlated. We referred this problem as overconditioning, namely multiple conditioning data within the correlation length. The way to resolve this problem is to treat the auxiliary information until they are no longer strongly correlated. Our implementation is to decimate the soft conditioning data, which is proved to be an effective method. However, the problem of overconditioing would still exist if we model the relationship between resistivity data and facies data using a different method but neglecting the internal correlation of the resulted soft conditioning data field. Therefore a more elegant approach to resolving the problem could be to re-code stochastic simulation part of TProGS so that it can deal with correlated soft data, e.g. collocated co-simulation, like the reviewer suggested. Then we could test how the decimation scheme works with other soft conditioning methods.

The aim of this study is to capture the geological model uncertainty, through a set of realizations of the subsurface facies model. In that context we believe to have succeeded, because our final set of realizations could be validated against five criteria.

Specific comments:
l.30: spatially correlated data locations? Unclear.

We mean the spatial correlation between the data locations, caused by multiple points per correlation length. The lines in the abstract have been changed to:

"This study integrates two distinct datasources into the stochastic modeling process that represent two extremes of the conditioning density spectrum; sparse borehole data and abundant SkyTEM data. In the latter the data has a strong spatial correlation caused by its high data density, which triggers the problem of overconditioing. This problem is addressed by a work around utilizing a sampling/decimation of the dataset, with the aim to reduce the spatial correlation of the conditioning dataset."

> *l.108: overconditioning: this is a concept that should be better discussed and analyzed (see my comment above)*

We believe that we have discussed overconditioing in TProGS (second point of the discussion above) adequately in our manuscript. As this is mainly an applied TProGS study we would like to keep the discussion focused on TProGS topics. Testing other soft conditioning methods for overconditioing goes beyond the scope of this study.

> *l.111: compatible: is it meant comparable?*

We agree and changed it accordingly.

> *l.139-141: The method of cross-validation is widely used and accepted. It might be mentioned here.*

We agree, but will not mention it at this point, because we already present four possible validation methods.

> *l.180: validated according to the method of...*

Changed accordingly.

> *l.241-243: Here you could argue in favor of your method that since the relationship is calibrated for this specific site, it should be valid for this same site and should incorporate many sources of uncertainty.*

Good point which we included in the revised manuscript.

> *Section 6.1: This section is mostly introduction material, and anyway it is a repeat of things already said in the introduction.*

This section was added during the first revision of the manuscript, because all reviewers uttered a lack of discussion on pros and cons on TProGS compared to other geostatistical methods. We believe that it supports the general discussion. However we agree that there is some repetition in it, so we shortened the section.

Section 6.3: What is done is essentially a kriging + thresholding operation. This typically results in a smoothed spatial structure, unlikely to be representative of the actual subsurface heterogeneity.

We agree, but still believe that despite inversion and kriging, the gridded SkyTEM dataset is still a reliable proxy for subsurface heterogeneity, especially at near surface where we find the finest resolution.

l.578: Even with a very small grid size, the smoothing will result in an artificial increase in the correlation lengths which will bias the results. It is not a question of grid size but a problem of different spatial correlation models used for the same variable.

We agree, but as stated above: The difference in spatial nature of the two variables is one uncertainty that is lumped with other uncertainties in the Histogram-Probability-Matching method.

l.602: This is a statement that is not very credible: given that the manuscript is about detailed features of the Tprogs software, the authors should be able to describe its functioning with certainty.

The sentence has been reformulated so that it sounds more definite now.

“Soft information is not considered correctly during the cokriging of the local probability estimate in the SIS step nor is it completely accounted for in the objective function used for the simulated quenching in the latest TProGS version.”

Section 6.6: Several other non-stationarity modeling methods exist. The described approach (zonation) is very crude. Trend-based modeling generally gives much better results and is in effect used since an auxiliary variable (SkyTEM) is used, which contains the non-stationarity information. Therefore I believe that this mention to zonation is not needed here and could be removed.

Good point. We decided to delete section 6.6 on “Non-stationarity” from the discussion, because it is already pointed out at several occasions in the results section that non-stationarity in terms of proportion and mean length gets simulating accordingly when vast soft conditioning is applied, even when the conditioning dataset is decimated. The discussion on zonation is redundant at this point.

l.673-676: These sentences are not useful and can be removed.

Paragraph was shortened.

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Author comment to review #2: Yoram Rubin.

We would like to thank Yoram Rubin again for reviewing our paper and are pleased that he acknowledges the scientific improvement after our revision. We also enjoyed the scientific dialogue and were contented for his thorough revision and thoughtful comments on our manuscript, which undoubtfully improved the scientific quality of our publication.

> *Dear Editor,*
>
> *There are several areas where I disagree with the authors' analyses and responses. I do respect the authors opinions and enjoyed the dialog. The nice thing about HESS is that the review comments are made available to the readers and so the reader can judge and decide for themselves.*
>
> *Respectfully,*
>
> *Yoram Rubin*